

EXPERIMENTS

ON THE

CAUSE OF HEAT IN LIVING
ANIMALS,

AND

VELOCITY OF THE NERVOUS FLUID.

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CHAP. I.

EXPERIMENTS ON THE CAUSE OF HEAT IN ANIMALS.

IN a treatise, published on the gout in April 1769, I ventured to deliver an opinion, relating to the nerves and the heat of animals, very different from any hitherto given. I there mentioned, that the substance which filled up the nerves, usually called the nervous fluid, was of an earthy quality, and descended through the nerves to the bones, where it constituted the ossific matter: that the motion of this earthy substance, in its way to the bones, raised a certain degree of friction with the sides of the little nervous tubes through which it passed, capable of exciting that degree of heat always kept up in the bodies of animals.

Ever since the opinion relating to the cause of heat in animals came into my mind, I have
B been

been endeavouring to discover a method of determining this point by experiment. Several months however passed away before I could fix upon the nature of the experiment. At last it occurred to me, that if the nerves were really the cause of heat in animals, every animal, deprived of the influence of a certain number of nerves, must at the same time be deprived of the heat which they contributed to the general heat of the animal.

To ascertain this, it was therefore necessary to destroy a certain number of nerves in an animal, and then to observe the consequences of the operation. Accordingly I collected a number of rabbits, and made various experiments of this nature upon them. But as fewer nerves were destroyed in some of these experiments than in others, I thought it would illustrate the subject to bring those, which bore the greatest affinity to each other, into one point of view.

EXPERIMENT I.

I applied the bulb of Fahrenheit's thermometer between the thigh and belly of a half grown rabbit, in order to ascertain the degree of heat in the animal: the mercury rose to 103° .

I then thrust an awl into the spinal marrow, near the articulation of the last rib, to destroy the nerves going to the hind legs. The rabbit immediately showed every symptom of exquisite pain, and its hind legs were strongly convulsed. When the convulsions went off, the legs seemed to be deprived of all power of motion, for they lay extended behind the animal in whatever position they were placed.

On the day following, the same thermometer was applied to the same rabbit, and to the same part of the body, and the mercury rose no higher than 100° .

On the second day after the operation, the mercury rose only to 98° . on the third day to $101\frac{1}{2}$.

On

On the third day I thrust in the awl a second time, below the first wound, to try if the nerves were as completely destroyed as the loss of motion seemed to indicate; and I observed the paralytic limbs to be as strongly convulsed in this operation as they had been in the first, although the rabbit never once manifested the smallest sense of feeling.

On the fourth day after the first operation, the thermometer was again applied to the groin of the rabbit, and the mercury was supported at 100° . on the fifth day at 102° . on the sixth I had no opportunity of measuring it; on the seventh day the mercury was carried to 100° . on the eighth to 99° . on the tenth to $98\frac{1}{2}^{\circ}$. on the eleventh to $99\frac{1}{2}^{\circ}$. on the twelfth to 98° . on the thirteenth to 91° . on the fourteenth to 93° . on the fifteenth the rabbit died.

This rabbit only lost the motion of the muscles of the hind legs, by the injury done to the spinal marrow. It ran upon its fore legs with great facility, and showed no sign of uneasiness.

easiness from the second day after the first operation. The muscles of the hind legs were however so emaciated that scarce any vestiges of some of them remained. It was indeed incredible that so great a waste could have happened in fifteen days to any animal that had plenty of food and a sharp appetite.

EXPERIMENT II.

I found the heat of a full grown rabbit to be 101° . I then thrust the awl into the spinal marrow, and, after the convulsions had subsided which this produced, the hind legs remained without any power of motion.

On the day following the heat was 98° . on the next day at 100° . on the third day at 99° . on the fourth day at $99\frac{1}{2}$. on the fifth at 100° . on the sixth $98\frac{1}{2}$. on the seventh $99\frac{1}{2}$. on the eighth 99° . The rabbit died on the twelfth of a palsey in one side.

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EXPERIMENT III.

I ascertained the heat of another rabbit to be 102° . I then destroyed the spinal marrow at the same place as in the first experiment, and the same appearances were produced.

On the day following the operation, the heat was 100° . on the second day 99° . on the third day 101° . on the fourth day 100° . on the fifth day 102° . on the seventh day 101° . on the eighth day $99\frac{1}{2}$. on the ninth day 101° . on the tenth day $97\frac{1}{2}$. on the eleventh 99° . on the twelfth 97° . on the thirteenth day 96° . The rabbit died suddenly on the fourteenth,

EXPERIMENT IV.

I ascertained the heat of another half grown rabbit to be 100° . I then thrust in the awl between the third and fourth vertebra of the back, and the hind leg became instantly paralytic.

On

On the day following the operation, the heat was $96\frac{1}{2}^{\circ}$. on the second day 96° . on the third day 97° . on the fourth $97\frac{1}{2}^{\circ}$. on the fifth $97\frac{1}{2}^{\circ}$. on the sixth $96\frac{1}{2}^{\circ}$. on the seventh $95\frac{1}{2}^{\circ}$. on the eighth 91° . on the ninth 89° . The rabbit died on the tenth greatly emaciated.

EXPERIMENT V.

I determined the heat of another middle sized rabbit at 96° . I then inserted the Owl between the second and third vertebra of the back, which took away the motion of the hind legs. In seven hours after the operation, the heat had diminished 21° , for the mercury only rose to 75° . The animal was at this time dosing. In twenty-four hours however the mercury was raised to $97\frac{1}{2}^{\circ}$. In fifty hours the heat was at 90° . on the fourth day at $92\frac{1}{2}^{\circ}$. on the fifth day at 96° . on the sixth at 95° . on the seventh at $91\frac{1}{2}^{\circ}$. on the eighth at $89\frac{1}{2}^{\circ}$.

on the ninth at $91\frac{1}{2}$. on the tenth at $92\frac{1}{2}$.
on the eleventh at $93\frac{1}{2}$. on the twelfth the
rabbit died.

EXPERIMENT VI.

I applied the thermometer to the groin of
another rabbit, and found the heat to be
 $101\frac{1}{2}$. I then thrust in the awl between the
first and second vertebra of the loins, and imme-
diately the hind legs became paralytic.

On the day following the operation, the heat
was $97\frac{1}{2}$. on the second day 99. on the third
day 97. on the fourth 98. on the fifth 97.
on the sixth $98\frac{1}{2}$. on the seventh $98\frac{1}{2}$. on
the eighth 98. on the ninth 95. on the tenth
95. on the eleventh 96. The rabbit died on the
twelfth.

EXPERIMENT VII.

I applied the thermometer, in the usual manner,
to another rabbit, and observed the mercury
supported

supported at 99° . I then wounded the spinal marrow about the middle of the bones of the back,

On the day following, the mercury in the same thermometer was carried no higher than 94° : at the same time the animal gave no symptoms of pain when the awl was thrust into the joints of the paralytic limb, it was however capable of walking upon the fore legs, and appeared to be very cheerful. The mercury was supported one degree higher when the bulb of the thermometer was inserted between the fore leg and thorax, than when inserted in the groin, the usual part of application.

On the second day the heat was 94° , and the rabbit still able to drag the paralytic parts of the body after it; on the third day 97° . and in good spirits; on the fourth day 93° . on the fifth 93° . on the sixth 96° . on the seventh 97° . The rabbit died on the following day.

I measured the heat of another full grown rabbit, and found it made the mercury rise to 100° . I then destroyed the power of motion in

EXPERIMENT VIII.

I applied the thermometer to a full grown rabbit, and observed the mercury carried to $101\frac{1}{2}$. I then took away the motion of the hind legs, and left the animal extended upon the ground, in an atmosphere of 52° .

On the day after the operation, the heat was 97° . on the third 95° , and the atmosphere was 55° . on the third $97\frac{1}{2}$, and the atmosphere 54° . on sixth day 96° , and the atmosphere 68° . on the seventh day $97\frac{1}{2}$, and the atmosphere 60° . on the eighth day 94° , and the atmosphere 60° . on the ninth 94° . on the tenth 97° . on the eleventh $95\frac{1}{2}$. on the twelfth 98° . on the thirteenth $98\frac{1}{2}$. on the fourteenth 96° . on the fifteenth $95\frac{1}{2}$. on the sixteenth 95° . on the seventeenth 95° . The rabbit died on the eighteenth.

EXPERIMENT IX.

I measured the heat of another full grown rabbit, and found it made the mercury rise to 100° . I then destroyed the power of motion
in

in the hind legs by a wound with an awl in the spinal marrow. On the day following the operation, the mercury was at 94° . on the third day 92° . on the fourth day $95\frac{1}{2}$. on the sixth day $95\frac{1}{2}$ on the seventh day 96 . and on the eight 96 . on the ninth $96\frac{1}{2}$. on the tenth $87\frac{1}{2}$. It died on the eleventh.

EXPERIMENT X.

I ascertained 97° to be the heat of another rabbit. I then wounded the spinal marrow about the middle of the bones of the back.

On the following day the heat had diminished 6° , for the mercury could be raised no higher than 91 ; it was raised however to 92 when the bulb of the thermometer was placed between the thorax and fore leg. The animal at this time supported the anterior parts of the body upon the fore legs, but had lost so many nerves by the operation, that it was unable to remove its hind parts from the place where they lay.

On

On the second day the mercury was carried 3 degrees higher than it had been on the first; on the third to 96. on the fourth 93; this day the rabbit began to drag the paralytic, which it had not been able to do since the operation. On the fifth day 93°. on the sixth 96°. on the seventh 97°, and the atmosphere much warmer. On the eight 93°. on the ninth 95°. on the tenth 94°. on the eleventh 94°. on the twelfth 93°. on the thirteenth I made no observation; on the fourteenth 92°. on the fifteenth no observation; on the sixteenth 93°. on the seventeenth 93°. on the eighteenth 91°. on the nineteenth the rabbit died.

All the rabbits in these ten experiments appeared sorrowful and inactive for a day or two after the operation. They then began to walk upon the fore legs, dragging the paralytic limbs after them. They were very thirsty for five or six days, but when the thirst went off, they never showed any other sign of uneasiness. From the time they began to walk, they eat with their usual appetite. All of

of them, which I happened to see die, expired suddenly; for they appeared to be as well a few hours before death as they had been on any day after the division of their spinal marrow,

Having broke Fahrenheit's thermometer, the four following experiments were made with one of less sensibility.

EXPERIMENT XI.

I observed the heat of another rabbit raise the mercury to 88° . I then thrust in the awl about the middle of the bones of the back, and immediately the hind legs became paralytic.

In twenty-four hours the heat was at 85° on the second day at 87° . on the third at 84° on the fourth at 81° . on the fifth at 79° . on the sixth day the rabbit died.

This animal lay flat on its belly, discharging thin feces, supporting the fore parts of the body on its fore legs, but unable to remove from the place where it lay.

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EXPERIMENT XII.

I found the heat of a full grown rabbit to be 88° . I then thrust the awl into the spinal marrow, about the middle of the vertebræ of the back.

On the day following I found the heat to be 16° . degrees less than it had been before the operation; on the second day 10° , and now the rabbit was much inclined to sleep; on the third day the heat was only $4\frac{1}{2}^{\circ}$ less, and the animal in good spirits; on the fourth day 3° . on the fifth day 3° . on the sixth day 4° . on the seventh day 5° . It supported itself on this day on its fore legs with more difficulty than usual, and was frequently tumbling on one side; on the tenth day it died in strong convulsions. The heat of the body, immediately after death, was 74° degrees; this was a greater heat than it had in the day after the operation.

I had

I had no opportunities of attending to the rabbit for three days before its death.

EXPERIMENT XIII.

I applied the bulb of the thermometer to the groin of another rabbit, and settled the heat of the animal at 89° . I then thrust the awl into the spinal marrow, near the articulation of the first rib, and the hind legs became paralytic. The stability of one of the fore legs was likewise impaired, but that of the other remained in its usual state.

The animal, being thus deprived of the powers of walking, lay extended on its side, very often struggling to rise on its fore legs and support itself on them; but although it was frequently assisted in these attempts, yet it could never maintain itself in that posture, unless it rested on the sternum, and even then, upon the first endeavours to motion, always turned over on its side.

Between

Between five and six hours after the operation, the same thermometer was applied to the same part of the body, and the heat was observed to have diminished 10° . At this same time the rabbit was much disposed to sleep, and continually nodding.

I applied the bulb of the thermometer to the rabbit at Twelve hours after the operation, the rabbit was so cold and inanimated, and to appearance so near death, that I imagined it would no longer serve the purposes of the experiments. I therefore did not apply the thermometer at this time. I however wrapped it in flannel, to try if it was possible to preserve the little life which remained through the night.

The animal, being thus deprived of the powers of working, in a few days after the operation, On the morning of the following day, the rabbit was found still living in the same torpid state, but I could scarce feel the motion of the heart.

I therefore exposed it before the fire, to a degree of heat, which in a sound state it would not have been able to bear, and the contractions of

of the heart were not only numerous but easily felt, the animal at the same time grew lively and active.

Thirty hours after the operation, the rabbit was able to stand upon its fore legs and appeared in good spirits, and began to eat. I then applied the thermometer again, and observed the heat to have diminished 16° . In the forty-fourth hour after the operation it died.

EXPERIMENT XIV.

I found the heat of another rabbit to be $87\frac{1}{2}^{\circ}$. I then destroyed the spinal marrow about the same part of the spine as in the last experiment. Immediately after the operation the rabbit lay in whatsoever position it was placed.

In twenty-four hours the mercury stood at 72° . and the heart beat 122 in a minute. In forty-eight hours the heat raised the mercury to 77° . and then the pulsations of the heart were 90 in a minute. The rabbit was now continually dosing, and died about the sixtieth hour.

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When opened, several livid spots were spread upon the stomach, which was likewise much distended with wind, although the rabbit never eat after the operation.

EXPERIMENT XV.

I measured the heat of another rabbit, of five weeks old, and observed the mercury was raised to 96° . I then thrust the awl into the spinal marrow, near the first rib. This operation took away the powers of motion from the hind legs, and also in a great measure from the fore legs, so that the rabbit lay extended in whatever situation it was placed.

In eight hours the heat was only 71° , and the pulse 100 in a minute. I then laid it down before a fire in a heat of 96° , and the pulse rose in half an hour to 137. In twenty-two hours the heat was 79° , and the pulse 120. I placed the rabbit in a heat of 108 , and in three quarters of an hour, the heart beat 183. I had, however,

however, no opportunity of measuring the heat of the rabbit from this time, because I was obliged to keep it lying before the fire, which preserved its life for two days longer.

EXPERIMENT XVI.

I observed the heat of another rabbit, of the same age with that in the last experiment, to be 97° . I afterwards destroyed the spinal marrow below and near the first rib. The animal immediately lay extended, being unable to remove from the situation in which it was put. It had however a little more strength in the fore legs than the last rabbit.

In six hours the heat had fallen 21° , for the mercury only rose to 76 . At the same time the heart beat 102 in a minute. I laid the rabbit before the fire, in a heat of 96° , and in half an hour the pulse was 162.

In twenty hours after the operation, the heat of the rabbit was 78° , and the pulse 122 in a minute.

minute. I then laid the animal in a heat of 108° , and the heart beat 200. I had no future opportunity of applying the thermometer, for the rabbit was always lying before the fire till its death.

EXPERIMENT XVII.

I observed the heat of another rabbit, of the same age with those in the preceding experiments, to be 98° . I then thrust in the awl between the first and second vertebra of the back, and all the legs of the animal instantly became paralytic.

In six hours after the operation, the heat had diminished 25° , for the mercury rose no higher than 73° . At this time the heart only made 72 strokes in a minute. I then laid the rabbit before the fire in a heat of 96° , and the pulse rose to 140 in a minute.

In twenty hours after the operation, the heat was 79° , and the pulse 118 in a minute. I now laid the rabbit before the fire, in a heat of 108° , and

and the heart in three quarters of an hour beat 180. I had no farther opportunities of observing the heat, being obliged to keep the rabbit lying before the fire, to preserve life as long as possible.

EXPERIMENT XVIII.

In an atmosphere of 54° , the heat of a dun coloured full grown rabbit made the mercury rise in Fahrenheit's thermometer to 100° . I then thrust the awl into the spinal marrow, below and near the first rib. The rabbit immediately lost all the powers of removing from the place where it lay.

The animal, on the day following, was found stiff, and almost inanimate. I therefore hastened to place it near the fire, which preserved life for that day. At night it was wrapped in flannel, together with another rabbit in the same situation. It was however so cold on the following day, that I imagined it could live but a few minutes, and therefore omitted applying the thermometer. On the evening of the third day, finding the rabbit

rabbit much recovered, I then applied the thermometer, and observed the heat to be no more than 77° , although it had lain before the fire most part of the day. The heart at the same time beat 127 in a minute.

In the middle of the fourth day, the heat was $76\frac{1}{2}$, and the pulse 147 in a minute. In the morning of the fifth day, the animal was found dead.

This rabbit had a continual shaking in the head, and eat very little after the operation.

EXPERIMENT XIX.

I determined the heat of a middle sized rabbit to be 98° . I then took away the motion of the hind legs in the usual manner.

In the morning of the following day, the rabbit was found stiff, and almost inanimate. To the touch, it was cold as the air itself. This made me hasten to lay it before the fire, by which

which circumstance, I was prevented from applying the thermometer for that day.

On the morning of the second day, the heat had however increased to 83° . At the same time, the heart beat 160 in a minute. On the third day, the heat was $84\frac{1}{2}$. On the fourth, $84\frac{1}{2}$, and the strokes of the heart 190 in a minute. On the fifth day, the heat was 80° , and the contractions of the heart so quick, as not to be accurately reckoned. The rabbit died in a few hours afterwards.

EXPERIMENT XX.

I observed the heat of another rabbit to be 99° . I then took away the motion of the hind legs, by thrusting an awl between the third and fourth vertebra of the back.

In the day following the operation, the heat was only 87° . on the second day 87° . on the third day it died.

This

This rabbit had no motion in the hind legs after the operation. The motions of the fore legs were likewise much impaired, but it eat several times between the operation and its death.

On the fourth day, the heat was 84° , and the strokes of the heart 120 in a minute.

EXPERIMENT XXI.
I took away the powers of motion from another half grown rabbit, after I determined the heat to be 97° .

In thirty hours the heat was only 70° , and the contractions of the heart 75 in a minute. In forty-eight hours the heat was 75° , and the strokes of the heart 120 in a minute. The rabbit died on the third day.

EXPERIMENT XXII.

I found the heat of another rabbit to be 100° . I then thrust the awl between the first and second vertebra of the back.

In

In fourteen hours the heat had diminished 30° , and the rabbit was so cold, that I was obliged to lay it before the fire during the forty-two hours which it afterwards lived.

The rabbits, in this and the last experiment, had lost the motion of their hind legs by the punctures. Their fore legs were likewise deprived of much power; for the animals lay in whatever situation they were placed. The rabbits never eat after the operation.

EXPERIMENT XXIII.

I ascertained $98\frac{1}{2}$ to be the heat of another young rabbit. I then divided with a knife the spinal marrow, between the second and third vertebra of the back.

On the day following the operation, the heat was only $83\frac{1}{2}$, the pulse 140. On the second day 70, and the pulse 70. I then laid the rabbit before the fire, in a heat of 85° , and in an hour

hour afterwards again reckoned the pulsations of the heart, which now amounted to 150. The rabbit died on the third day.

EXPERIMENT XXIV.

I determined the heat of another young rabbit at 97° . I thrust an awl between the second and third vertebra of the back, which destroyed the motion of the hind legs.

On the day following, the heat was 90° . on the third day after the operation, the heat was 82° , and the strokes of the heart 166 in a minute. The animal was at this time in a lethargic state, and had lost the use of all its legs. I then laid it before the fire, in an hour afterwards the heart beat 200 strokes in a minute. The animal died on the following day.

EXPERIMENT XXV.

I observed the heat of another rabbit to be $98\frac{1}{2}$. I then thrust an awl between the first and

and second vertebra of the back, and the hind legs of the animal became immediately paralytic.

On the day following the operation, the heat was only $71\frac{1}{2}^{\circ}$, and the pulsations of the heart 90 in a minute. I laid this rabbit before the fire, in a heat of 85° , and in an hour afterwards, the heart beat 160 times in a minute. The following night the rabbit died.

EXPERIMENT XXVI.

I intercepted the powers of motion in the four legs of another rabbit, by destroying the spinal marrow near the first rib. I had first observed the heat of the animal to be 99° .

The day following, the heat was only 70° , and the contractions of the heart 64 in a minute.

On the second day, the heat was 75° , and the contractions 130 in a minute. The rabbit died on the third day.

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hind and fore legs of the animal became immediately paralytic.

EXPERIMENT XXVII.

I found the heat of a middle-sized brown rabbit to be 101° . I then thrust an awl into the spinal marrow at the first rib, which not only destroyed all the motions of the muscles of the hind legs, but also much impaired those of the fore legs; for the animal was extended on its side, unable to remove from the place where it lay.

In seven hours, the heat of the rabbit had diminished to 100° . As it was very cold and motionless, I laid it before the fire, in a heat of 90° , and then the fore legs became quickly so firm and active, that they could bear the body forwards, when the hind legs were supported, so that the rabbit appeared to be re-animated. As it had received no benefit from the fire in the night, on the morning of the following day, which was 24 hours after the operation, the heat of the rabbit was only 107° , and it was apparently as strong as at the last application of the thermometer.

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On the third day after the operation, it began to eat, which it had not done for the preceding 48 hours. I was, however, obliged to keep it in a heat between 80° and 90° all the third day, and could not apply the thermometer on that account.

On the morning of the fourth day, it was quite stiff, and without any motion. I held it before the fire, and rubbed it, and put one foot into the fire before I was certain whether any life remained. It was then left before the fire, and again began to eat, but it died in the following night.

EXPERIMENT XXVIII.

I observed the heat of a middle sized white rabbit to be 100° . I then thrust the awl into the spinal marrow, nearer the tail than in the last experiment, and the hind legs became paralytic.

In seven hours, the heat had diminished 6° , in twenty four hours 16° . The rabbit now began to eat, which it had not done since the operation. It died, however, on the third day.

ALL

On the third day after the operation, it began to eat, which it had not done for the preceding 42 hours. I was, however, obliged to keep it

ALL the rabbits, in the last 16 experiments, were seized with a degree of stupor and grinding of the teeth after the operation. The stupor generally went off when the animals were laid before the fire. They were likewise observed to be frequently shivering in the parts of the body which retained their sensibility. In some of the rabbits, the shivering might be seen; in others, it could only be felt.

Sixty rabbits, at least, died in making the last 18 experiments, before the purposes of the experiment could be answered. Some died in the operation from accidental wounds; but most of them between the 20th and 30th hour after the operation:

The punctures, made in the spinal marrow of the rabbits which died soon after the operation, were not higher in the spine than those made in the rabbits which lived three days after it. Two animals indeed, injured in similar ways, were never

never alike affected by the injury, so differently did an equal violence affect the nerves.

The rabbits, in all the experiments, by losing the nerves of the muscular fibres surrounding the bladder, lost the power of expelling their urine. I was therefore under the necessity of compressing that part of the belly over the bladder, to compel the urine to run out; for in some of the first experiments, where this was neglected, the bladder burst, and the urine was evacuated within the abdomen.

The pulse of the rabbits in the first 10 experiments were not sensibly affected. That of the rabbits in all the other experiments was much less frequent.

Having now given a faithful account of every thing that occurred during the course of my experiments, I think it necessary to mention, that there was scarce more than a drop or two of blood spilt in any of the operations.

In all the foregoing experiments, two things have been constantly observed. First, The rabbits

bits became colder when the connexion between the nerves of the lower extremities and the brain was intercepted. Secondly, That the degree of cold was nearly in proportion to the number of nerves destroyed.

Since then all these rabbits became cold by a loss of nerves, and suffered a diminution of heat, nearly proportionable to the number of nerves destroyed, may it not be concluded, that the nerves are the means by which heat is conveyed throughout the bodies of animals.

CHAP.

tion of the different appearances which happened to some of these animals. Thus lived many hours after the operation; for some were colder than others, though their spinal marrow was divided at an equal distance from the brain. In the first and weakest experiment, for instance, the diminution of heat was much greater in the first twenty four hours after the operation, than in any of the other animals which walked upon their fore legs.

CHAP. II.

THE cause of heat in animals being once known, many difficult problems in physiology may be easily solved. I shall not however, at present, enter into the discussion of these several problems. I shall only take notice of a few observations which occurred during the course of the experiments, and make some obvious deductions from them.

It may be remembered, that the same injury done to the spinal marrow killed some rabbits thirty hours sooner than others. This difference in the sensibility of the nerves explains the rea-

son

son of the different appearances which happened to some of the animals that lived many hours after the operation ; for some were colder than others, though their spinal marrow was divided at an equal distance from the brain. In the fifth and twelfth experiment, for instance, the diminution of heat was much greater in the first twenty four hours after the puncture, than in any of the other rabbits which walked upon their fore legs.

The great cold, brought upon the animals of the fifth and twelfth experiments, appeared to proceed from the sympathy which the nerves, not divided by the puncture, shewed for those which were divided. The progression of heat was interrupted in the divided nerves. The progression of heat must therefore have been much diminished in the others ; but as the heat of the animals returned in a great measure on the second day, it was evident, that the heat at that time moved through the sound nerves with its usual velocity.

Although the cold brought on by the sympathy of the nerves, immediately after the operation,

tion, was most remarkable in the fifth and twelfth experiments, it was likewise observed to prevail on the day following the operation, in a less degree, in some of the other rabbits. The nerves of these animals which remained sound must therefore have, likewise, given less heat than they usually gave before the animal was injured. When the degree of cold was unusually great on the day following the operation, this could not be owing to the destroyed nerves alone, but in part proceeded from the sympathy of the sound nerves; and, in that respect, exactly resembled the cold shivering always observed to be the prelude of disease,

As the cold fit went off, the animals became nearly as hot as they had been before the operation. This increase of heat, coming after the cold fit, resembled the heat which attends fevers. Like animals in fevers, these rabbits were likewise very thirsty.

The nerves, not injured by the operation, must therefore have produced greater quantities of heat than they usually did; so that as this thirst continued from the second day after the operation

tion to the death of the animal, the heat must be allowed greater all that time than it would otherwise have been.

It may be necessary to observe, that there was a general coldness in the whole body of the rabbits after the operation, and that the paralytic parts were not more than one degree colder than the parts not paralytic. This proceeded from the action of the heart carrying on the circulation, and propelling the blood into the vessels of the paralytic parts, after it had been heated in the parts where the nerves were not destroyed. By these means the paralytic parts were maintained in a degree of heat very little inferior to that of the moving parts of the body.

Paralytic parts of animals being colder than parts not destitute of motion, the blood shall be therefore hotter upon entering the vessels distributed to paralytic parts, than on running out of them. If the arm is paralytic, and all the other parts of the body preserve their natural powers, the blood, on entering the humeral artery, shall be hotter

hotter than the same blood in its return through the humeral veins.

In partially paralytic animals, the blood is continually heating the paralytic parts, and the paralytic parts are in a reciprocal proportion continually cooling the blood. The whole blood of such animals will be therefore colder than the blood of the same animal before any of its parts were rendered paralytic; for the cooled blood, in returning from the paralytic members, mixes with the warm blood coming from the parts not paralytic; by which mixture the heat of the whole mass will in time necessarily be diminished.

Altho the motion of the hearts of the rabbits in the last sixteen experiments, was far from being proportionable to the heat of the animals after the operation, yet that organ was always observed to be in a great measure affected by the condition of the rabbits; for when the rabbits were exceeding cold, the motion of their hearts was scarce to be felt; on the contrary, when the animals were laid before the fire, and had received

ceived a degree of adventitious heat, equal to the natural heat which they lost by the destruction of their nerves, their hearts quickly recovered new powers, and the rabbits during that time became re-animated.

The slow motion of the heart, which attended the great degree of cold in these animals, appeared chiefly to be owing to the following cause: Almost all the nerves going to the muscles, and to the various vessels of their bodies, had been divided at the first rib. The connexion of their nerves with the brain being destroyed, no farther supplies of heat could be transmitted through them. All the parts, to which these destroyed nerves were distributed, being thus deprived of heat, gradually became colder and colder. As the circulation still continued to prevail in these parts, the blood likewise became gradually colder in its passage through vessels unusually cold. Heat in the blood being however the great stimulus to the motion of the heart, whenever that fluid loses any degree of heat, its stimulating power is abated in the same proportion. When the blood therefore of these

these animals was cooled in the paralytic parts of their bodies, and carried back through the veins to their hearts, it was less capable of compelling these organs to make quick contractions; and as fresh supplies of colder and colder blood continued to be returned, the contractions became less and less frequent; at last the contractions were so very slow and feeble, as to be unable to propel the blood with sufficient momentum into the brain, for the purposes of the nerves descending to the heart itself, and then the animal died.

I attended to the motion of the hearts of some of the animals immediately after the operation, to observe whether the contractions had become slower by the division of the spinal marrow; but I was always of opinion, that the heart rather beat quicker and stronger at these times. The diminution of irritability, observable in that organ some hours after the operation, did not appear to have been occasioned by any sympathetic injury which the cardiac nerves received during the operation, otherwise this injury would have been manifested immediately; but appears
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first to have proceeded from an influx of colder blood. It was evident that the cardiac nerves afterwards transmitted lesser quantities of heat when the powers of the heart became very much impaired.

In this manner does a slow motion of the heart generally follow, and attend diminution of heat in the bodies of animals. Cold in animals is not however produced by the slow motion of the heart; the slow motion of that organ being only the consequence of cold, or a symptom that cold prevails in the animal.

It may be remembered, that many of the rabbits were laid before the fire in this torpid state, and then the contractions of their heart became both more frequent and strong.

It has been long known, that heat compels the heart to quicker contractions. The quicker motions of that organ in these animals may have proceeded from two causes; for warm blood is not only a better stimulus than colder blood; but heat renders also the heart itself more susceptible of irritation. It was impossible however

to ascertain the quantity of stimulus in the one, or the degree of improved sensibility in the other; for the nerves of the heart could not receive the influence of the fire, and every other part of the body be exempted from that influence; neither could the various parts of the body be exposed, and the nerves of the heart be at the same time defended from it; neither can the increased contraction of the hearts of the rabbits, lying before the fire, be attributed to the heated blood, nor to the greater irritability of the heart alone, but might proceed from both these causes. It is however easy to see, that although two causes may have been concerned in exciting this phenomenon, one of these causes was sufficient to have produced the same; for if the nerves, which are distributed to the various parts of the body, may transmit more heat than usual, and the nerves of the heart at the same time transmit lesser quantities; the animal shall have a hot skin without an increase of pulse. If the nerves descending to the heart carry greater quantities of heat into that organ than usual, and all the nerves in the other parts of the body transmit lesser quantities,

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quantities, an intense cold shall come up on the animal, notwithstanding the contractions of the heart shall be very frequent. When the nerves going to the heart, and those distributed throughout the body, concur in transmitting an uncommon degree of heat, the animal will then have a burning fever. When all the nerves of the body cease to transmit heat, then shall the animal die in a moment.

CHAP.

CHAP. III.

EXPERIMENT ON THE VELOCITY OF THE NERVOUS FLUID.

I Frequently had occasion, during the course of the foregoing experiments, to thrust the awl into the vertebræ of the rabbits below the puncture made in the spinal marrow, with a view of being certain whether the nervous connexion between the lower extremities and the brain was completely intercepted. In all the trials of this kind which I made, I constantly observed, that the paralytic limbs of the animals were strongly convulsed, and yet the animals appeared not to feel the injury; which proved, that the communication between the marrow below the first operation and the brain was totally cut off.

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These second punctures, in the spinal marrow, were not always made on the same days in which I made the first punctures, but were sometimes deferred for five or six days after, and then the paralytic legs of the rabbits were as strongly convulsed as when these second punctures were made on the day of the first operation. In other rabbits, immediately after I had divided their spinal marrow, I thrust the awl into the next joint of the vertebræ below the division, and afterwards into the second joint below, and so on to the tail of the animal, and fresh motions were constantly excited in their paralytic legs, without any symptom of sensation being given by the rabbits. By these facts it seemed that the motion of the nervous fluid was not so very quick as has hitherto been imagined. In order to be certain whether this was really the case, I made the following experiments:

I divided with a knife the back bone and marrow of a rabbit at the lowest rib: I then irritated the divided extremity of the marrow going to the tail, having no communication with the brain, and immediately the hind legs were strongly

strongly convulsed. The irritation was continued till no further motion could be excited. The rabbit never shewed any sign of uneasiness after the division of the marrow; I then irritated the divided extremity of the marrow that was connected with the brain, and although the rabbit immediately expressed a sense of most acute pain, no convulsions were raised in the hind legs, but they remained perfectly paralytic.

I repeated the same experiment upon another rabbit, and the same appearances were produced.

On the day following these operations, I thrust the awl into the same wound of the back, and again irritated the extremity of the marrow that went to the tail, but no convulsions could be raised in the hind legs of any of the animals: I then thrust an awl into the first joint of the back below the wounds, and strong convulsions were raised in the hind legs.

Now, if the substance which filled up these nerves, moved so instantaneously as has hitherto been imagined, it ought of necessity to have been

been compelled to have run out of the nerves below the division, by the great irritation which the knife produced. But it appears that neither the injury which the marrow sustained in the division of it, nor all the future irritations made by the awl upon the divided extremities of the marrow, could compel this nervous fluid to descend further down in the nerves than the distance of the next joint in the back.

As I imagined the nervous fluid was not driven down the marrow so low as the next joint from the irritation, and as I found some difficulty in perforating the solid bone between the articulations, I determined to lay bare a large bundle of nerves in some of the fleshy parts of the body, and make more accurate trials of the same nature upon them.

I accordingly tied down a rabbit upon its belly, with its four feet extended, and made an incision through the muscles of its buttock to a large trunk of nerves issuing from the lower end of the spine. I then tied the nerves, and set at liberty the leg of the animal to which the
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tied nerves were distributed, and I irritated the
 nerves above the ligature, but no motion was
 raised in the leg. I took hold of the nerve be-
 low the ligature with a pair of small forceps,
 and the usual motions were excited in the leg.
 The animal shewed no sense of pain by irritating
 below the ligature, but shewed greater pain
 when the nerve was touched with the forceps
 above the ligature. I then divided the nerve
 at the ligature, and began to irritate the divid-
 ed nerves that had lost connexion with the
 brain. I took hold of the nerves with the forceps
 as near their extremities as possible, and gently
 squeezed them till motion appeared in the muscles
 below. After a moment's interval, I renewed
 the pressure upon the very same parts of the
 nerves, and motion again appeared in the mus-
 cles, I repeated the pressures, after like intervals,
 for thirteen times successively, and the same mo-
 tions were excited thirteen times in the muscles;
 no further pressure in that part could then renew
 the motion. I therefore removed the forceps
 a little space down the nerves, the space did not
 exceed the breadth of two lines, but when I re-
 newed the pressure upon this new point, fresh
 motions

motions again appeared in the muscles. Motions were only excited three times by pressing the nerves in this spot. I then carried the forceps the same small distance down the nerves as I had done before, and motions were renewed in the muscles. These motions were more numerous than they were in the last instance. In the same manner did I descend an inch and half of nerve, pressing the nerve in every point of that distance successively, and continuing the pressures in every point till no motions could be renewed in the muscles; when it was evident that either all the fluid in the nerves had been driven down from under the forceps towards the muscles, or else the nerves, which is much more probable, were so injured by the repeated pressures, as to be disqualified from propelling their fluid. Two other nerves were irritated with the same deliberation, and the very same consequences followed.

The muscular motions, excited by driving the nervous fluid out of one inch of nerve, appeared to me to be as numerous as the ordinary exercises of the animal would have required in
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one day, and I am convinced that the impulse of the will of the animal acting upon the extremities of these nerves in the brain, would not have driven forward the nervous fluid half an inch in performing the same motions. Admitting however the first calculation; that the nervous fluid, in its way from the brain, does really advance an inch in one day through the nervous tubes; in eighteen days it shall only traverse eighteen inches, which was nearly the distance between the cerebellum and feet of the hind legs of the rabbits, consisting of thirteen feet to the point of division, and five feet to the other end. If the nervous fluid, in the nerves of rabbits, only moves at the rate of one inch in twenty-four hours, and the fluid in human nerves be allowed to move with the same velocity, then the same element of nervous fluid that enters one end of a nerve in the brain, shall be seventy-two days in passing through a nerve of the length of six feet.

From the last experiments it appeared, that the nerves which had lost connexion with the brain, had the same powers of giving motion, as

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they had before they lost connexion, and that these powers proceeded from the divided nerves remaining filled up with fluid after the division.

It will readily be allowed, that a nerve that can be discovered to have been full of fluid after the division, must have been full of fluid before it was divided. The nerves in a healthy state, are therefore always filled up with fluid. When an irritation is made upon the extremities of divided nerves filled with fluid, the fluid is put in motion at the point of irritation, and the motion communicated throughout the fluid to the other end of the tube spread upon the muscle, but the fluid that was first touched by the irritation, remains nearly in the state it was; if this was not the case, motions could not be twice raised in muscles, after the nerves going to them had lost their connexion with the brain, for all the nervous fluid would be driven out of the nerves at the first irritation, and then the nerves being empty, and receiving no farther supplies from the brain, could not renew any motions in the muscle.

muscle by a second irritation. This however, is contrary to experiment. The nervous fluid was not driven out by any degree of irritation, continued in one part of the nerve. The fluid appeared to pass the nerve with such difficulty as scarce to remove from the force impelling it. So that when I wanted to drive the fluid out of the nerves, I was obliged to make the irritation lower and lower in the nerve, and then could not compel the fluid to descend the nerve the breadth of a hair from the point I touched; the fluid and nerve had such a close attachment.

From the tardy motion of the substance which fills up the nerves, it would appear, that the substance has not that high degree of fluidity hitherto attributed to it; that the substance is rather of a compact quality, approaching to solid, but capable of giving nutrition to the nerve in which it moves; that a nerve, filled with this substance, seems to resemble a flexible cylindrical tube accurately filled with a flexible cylindrical rod. When one end of the rod is driven forward, the impulse conveys instant progression

progression to the other end. So that when one end of the nervous substance is impelled by the mind, in the brain, the other end in the extremity of the body is instantly driven forwards. The particle of nervous matter, which receives the impulse from the mind, does not traverse the whole length of the nervous tube at a flight, to carry motion to the other end of the nerve; for by the preceding experiments, as the particle of nervous matter impelled by the forceps, did not go on the breadth of a hair by that impulse, neither can the particle of nervous substance impelled by the mind, possibly remove to a sensible distance from the vital power which strikes it, but must necessarily remain, after it receives the impulse, nearly in the same situation which it possessed before the impulses.

I made two experiments of the following nature, to discover the manner in which the soul of the animal accelerated the motion of the substance in the nerves.

I laid

I laid bare a large bundle of nerves issuing from the lower end of the spine of a rabbit. I then made repeated strokes upon the nerves, in the same manner as a person strikes who wants to make a cord vibrate. The animal did not express any pain from the stroke, but some of the muscles, to which the nerves were distributed, went into faint motions: I squeezed the nerves between my fingers and thumb with the utmost force, but the animal felt no pain, neither was any motion raised in the leg. I then divided the nerves, and held the divided extremities that went to the leg between my fingers and thumb, to support them in a sufficient resistance to the irritation which I intended to make directly upon the ends of the divided nerves. I struck the ends of the nerves supported in this manner with a probe, imagining that the direction of the stroke was most favourable to drive forward the nervous substance, and stronger motions appeared in the muscles then when the nerves were struck laterally, although the nerves gave way to the stroke, and could never be maintained in firm resistance.

I touched

I touched with a gentle pressure the divided end of the bundle of nerves that went to the brain, and the animal felt much pain. I struck the side of the bundle with a considerable force, but no complaint was made by the animal.

I took hold, with the forceps, of the extremities of the nerves that had lost connexion with the brain, and stronger convulsions were raised in the muscles than had been excited when the divided ends of the nerves were struck with the probe. I squeezed the extremities of the nerves that continued in connexion with the brain, and exquisite pain was given the animal.

A considerable pressure was made upon the divided nerves with the forceps before motion could be excited in the legs. A lesser degree of pressure, made upon the nerves going to the brain, sensibly affected the rabbit. The least degree of pressure made by the probe upon the extremities of the nerves, that tended to drive back the nervous substance towards the brain, was equally felt with great pressure upon the sides of the nerves with the forceps.

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From these experiments it clearly appears that sensation is not carried on by vibration, for when the bundle of nerves were strongly made to vibrate, the animal shewed no symptoms of pain, and scarce any degree of motion appeared in the muscles to which the nerves went.

From these experiments it would also appear, that the vital power existing in the brains of animals does not compel the nervous fluid to move through the nerves by squeezing their extremities; but that it strikes at the end of the nervous tubes with a stroke so well defined, as to communicate impulse and motion to the substance in the nerves, without affecting the nervous tube containing the substance.

It has already appeared, that the nervous substance does not remove to a sensible distance from the impulse which the soul of the animal makes upon it. This short progression of the substance points out, that the vital power, has] either a very confined residence in the brain, or that the impulses of the power are very languid.

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The convulsions raised in the hind legs of animals, by dividing and irritating the spinal marrow, were not stronger than the struggles exerted by the natural powers of the animal. They were only more numerous, because I followed the nervous substance as it descended the nerves with repeated irritations. The short progression which the nervous substance makes when impelled by the mind, is therefore not owing to want of energy in the vital power of the animal, but to the limited stroke of the vital power.

It may be remembered, that the nervous substance never descended the nerve without irritation and compulsion, and that immediately after the irritation was removed, the substance seemed to rest in the nerve. From this circumstance it would appear, that it meets with the utmost impediment in its progress through the nerves; that, on that account, great attrition must attend the progression of it; so that the heat, which the nerves transmit, seems to arise from the friction excited by the motion of the nervous substance through the nerves.

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From the slow motion of the nervous substance, I observe, that the hardness existing in muscles during their state of action, is not owing to an inflation of their fibres by nervous fluid; for as several irritations made on one spot of nerve, scarcely drove on the nervous substance a sensible distance from the point of irritation, and as every one of the irritations excited motion in the muscles, the quantity of nervous substance, necessary to perform one muscular motion only, must be exceeding small, and totally insufficient to fill one of the fibres of a muscle, admitting the fibres to be hollow, and of as minute diameters as the very nerves themselves.

When an animal wills to move a muscle, it accelerates the motion of the substance in the nerve going to the muscle, and heat attends the acceleration. The instant diffusion of heat throughout the fibres compels the elements composing them to approximate, in the same manner as wet cat-gut shortens before the fire. As the elements of the fibres approximate,

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the muscle moves, shortens, and grows hard. If this be the way in which muscular motion is performed, a muscle must always be preserved in a certain degree of moisture; moisture appearing to be as necessary to the motion of a muscle, as the nerves themselves.

The arteries, distributed throughout the muscles, deposit moisture for this purpose, for a glutinous fluid is always swimming in the cellular membrane which surrounds the muscular fibres. *Arteriæ nihil omnino conferunt, nisi quod integritatem musculi & mutuum partium habitum conservent, vaporem adipemque secernant & nutrant,* says the illustrious Haller, when speaking of the use of the arteries to muscles, § CCCCVI. *Prim. Lin.* This fluid now appears to be of the utmost consequence, and the means of conveying heat to the blood of animals.

When the fibres of muscles are heated by the nerves, the fluids in the cells surrounding them must be likewise heated, in the same manner as a hot solid, immersed in water colder
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than itself, heats the water. The fluid, thus heated by contact with fibres hotter than itself, is sucked up by the veins, and conveyed to the red blood, where it diffuses heat to all the globules of that fluid with which it comes in contact,

Every fibre in the muscles, endowed with nerve, shall contribute in a certain degree to preserve heat in the fluids of the animal. The quantity of heat which a fibre contributes, shall be in proportion to the extent and action of the fibre. A muscle in time of action, shall in general give more heat to the fluid in the cells surrounding its fibres, than during its state of rest. Muscles of great power and quick action, shall heat more of this fluid than others of lesser power and fewer actions.

The perpetual action of the muscular fibres of the heart, arteries, lungs, intercostal, and abdominal muscles, and other muscular fibres concerned in respiration, must necessarily be the means

means of heating much more blood than the muscles of voluntary motion.

It is impossible to ascertain the quantity of heat which the muscles of voluntary motion produce to the general heat of the blood of a rabbit; for where the spinal marrow of the animal was divided, and the nerves going to the muscles of the hind legs had lost connexion with the brain, the substance in these nerves being then capable of exciting mechanical heating effects upon the fibres of the paralytic muscles, and having a continual stimulus from the impetus and heat of the blood passing through the arteries of the paralytic muscles, it was highly probable, that the substance made a certain progression through the nerves during the life of the animals, and gave some degree of heat to the fibres of the muscles. The nerves of the rabbits which the operation had not injured, at the same time appeared to transmit greater quantities of heat than usual. At a medium; the animals in the first 12 experiments lost 3° of heat, by the destruction of the nerves
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of their loins and hind legs. If one degree of heat be allowed to have been produced by the action of the substance which remained in the paralytic nerves, and three degrees for the increased febril heat transmitted through the sound nerves, the nerves of the loins and hind legs of a rabbit shall only contribute 7° to the heat of the animal, and yet these muscles have greater proportioned powers in this species of beast, than in any other species. As the muscles in the fore legs have weaker powers, 4° may be a sufficient allowance for them. The muscles of the neck may contribute 2° . The muscles of mastication and deglutition, and all the other little muscles about the head 3° . 16° shall then be the share of heat contributed by the muscles of voluntary motion, to the blood of a rabbit of the heat of 98° . 82° must be therefore produced by the spontaneous organs of the animal.

Altho' many of the muscles of voluntary motion are frequently in a state of rest, yet the substance in the nerves going to them has a continual progression

progression at that time; for any impediment in the substance which fills the nerves, gives pain to the animal, sensations approaching to pain are often produced by very long inaction in muscles, but the action of the mind upon the nerves overcomes the tendency to rest in the nervous substance, and by quickening the progression of it, removes the disagreeable sensations in the nerves. If pain is seated in the muscles of the body, under the influence of the will, an exertion of the mind upon the nerves going to the muscles is the best remedy. The uneasiness therefore which arises in the voluntary muscles, shews that the motion of the nervous substance in the nerves is very liable to stop, unless it is promoted by a stimulus from the brain.

Although the muscles of voluntary motions in a rabbit may give 16° , and the spontaneous organs 82° , yet there is a reciprocal dependence upon the produce of each class of muscles; for if a number of the nerves of voluntary motion were destroyed, as happened in the last 16 experiments, the free diminution of heat from this

this loss, quickly lessens the produce of heat in the spontaneous organ; so that the spontaneous organs can seldom produce 82° , unless the nerves of voluntary motions transmit 16° . except there be a local deficiency of heat when the other nerves of the body transmit greater quantities than usual.

Seeing the spontaneous organs contribute so great a share of heat to the blood of animals, it will easily appear, that an increase of heat must always attend increasing powers in the heart and arteries; for as a strong exertion of a voluntary muscle is effected by a greater influx of heat than a lesser influx, more blood shall be heated in the heart by a strong contraction of that organ, than by a weaker contraction.

If the heart contracts 75 times in a minute, the fluids extravasated among its fibres from the coronary arteries shall receive 75 little heats in a minute, in one of the cavities of that organ only; but as the heart has four cavities, each of these shall give a quota of heated blood 75 times

times in a minute, in their act of contraction, in proportion to the number, extent, and action of the fibres furrounding the cavities. A heart, which contracts 75 times in a minute, will therefore contribute 300 little heats every minute to the blood of the animal. If the heart of the same animal should be compelled to give 15 contractions more of equal power in the same time, from a quicker descent of heat through the cardiac nerves, heated blood will not only be sent 15 times in a minute oftener out of the heart, but all the nerves in the various parts of the body, which feel the motion of the arteries, shall be solicited, 15 times in a minute oftener, to bring new supplies of heat from the brain.

The blood is not only heated among the fibres of the heart, in a lymphatic state, in the same way as takes place in other muscles, but it also receives a degree of heat in the state of red blood from contact with the muscular fibres furrounding the cavities of that organ. It would indeed appear, that red blood was often pressed
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out at the extremities of the arteries, distributed to all the muscles; during great exertions of the muscles, and that this extravasated blood gave the fibres in muscles that red colour with which they are always observed to be clothed.

The motion of the hearts of animals must now be admitted to be occasioned by the influence of the cardiac nerves alone; for as irritation made upon the nerves going to the paralytic muscles of lesser irritability than the heart, excited motions in these muscles several days after they lost their nervous connexion with the brain, it is evident, that the substance in the nerves is not rendered incapable, by remaining in a paralytic muscle for several days, of renewing its usual mechanical action upon the muscle, whenever that action is called forth by proper stimulus. The palpitations which continue in the hearts torn out of animals, and the motions observed to be renewed by irritations made on them some days after separation from the body, may all proceed from the mechanical action of the nerves upon these organs; for the many

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convulsions

convolutions of the fibres of the heart are well adapted to preserve the nerves in a fitness for exerting their usual effects upon the fibres, were the nerves liable to suffer from an inclement situation. But this is a matter not yet ascertained.

We know that moisture is absolutely necessary to produce motion in muscular fibres. It does not appear, however, that moisture is absolutely necessary to promote the progression of the substance in the nerves. It only appears, that it is absolutely necessary for the nerves to produce motion in the fibres, that the fibres should be moist. We know therefore, what disqualifies a fibre from being acted upon by a nerve, but are unacquainted with the degree of injury which a nerve may sustain before it be disqualified to act upon a muscle. The nerves may therefore be capable of enduring more inconveniency than the muscular fibres themselves. This however is certain, that as many motions can be raised in muscles by a small advance of nervous substance in the nerve, the heart of an animal

animal might contract several days without interruption, after taken from its body, and all these contractions be insufficient to consume the substance of the nerves distributed throughout the fibres of that organ.

Lastly, from the slow motion of the substance in the nerves, it appears, why the substance has never yet been seen to issue from divided nerves.

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animal might contract several days without its
resurrection, after taken from its body, and all
these contractions be sufficient to continue the
existence of the nerves distributed throughout
the fibres of that organ.

Lastly, I am now to show the
place in the system of the
nerves has been taken from the
which network



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